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HARMON NORTHRUP MORSE

AFTER a long life spent in service in Johns Hopkins University Professor Morse died September 8, in the seventy-second year of his age. He was born October 15, 1848, at Cambridge, Vermont, the son of a farmer, and died at Chebeague, Maine, where he had spent his summers for many years. He was graduated from Amherst College in 1873, then went to Göttingen, and received the degree of Ph.D. from that university in 1875. The year 1875–76 was spent at Amherst as assistant in chemistry. In 1875 it was announced that the Johns Hopkins University would begin its work in the year 1876. Shortly after it became known that the writer of this notice was to be the professor of chemistry in the new university he received a call from Morse who brought a letter of introduction from Emerson. This letter led me to take more than ordinary interest in the bearer. Whatever we were to do in Baltimore, it seemed clear that I should need an assistant, and I told him I would in due time arrange for his appointment. Hearing a little later of the fellowships that were to be awarded I secured one of these for Morse and so his connection with the Johns Hopkins University began. Before the doors were opened, however, he was designated associate, and we began our work together for better or for worse. We had no laboratory. We had less than a handful of students. What was to come of it? I need not go into the story, thus suggested except to say that we were absolutely untrammeled and left to work out our own salvation. Morse and I were of one mind as to the object to be attained and there were no discussions in regard to the methods to be adopted. They were not original, but they had never been tried in this country. There had never been an opportunity. The opportunity that many of us had hoped for, had dreamed of, was furnished by the bounty

of Johns Hopkins and the wisdom of his trustees and of President Gilman.

Morse remained an associate until 1883, when he became an associate professor. In 1892 he was promoted to be professor of inorganic and analytical chemistry, and in 1908 he became director of the chemical laboratory. In 1916 he withdrew from active service and became professor emeritus.

From the beginning of our work in the new university the importance of research was emphasized. That was indeed its most characteristic feature. Morse was as anxious as any of us to take part in this work. For one reason and another it was some time before he got going. To be sure he did show his hand in some small and rather unpromising pieces of work and I think he became discouraged, but he was faithful to his teaching. Gradually, however, his researches opened up new fields and he began their exploration. This is not the place for a full review of his contributions, and those of his last years so overshadowed all that preceded that a reference to those alone will do substantial justice to his memory.

In the early nineties he turned his attention seriously to the question of the stability of solutions of potassium permanganate and in 1896 he published an article on "The production of permanganic acid by manganese superoxide," A. J. Hopkins and M. S. Walker appearing as joint authors. Pursuing this subject Morse and H. G. Byers in 1900 published an article "On the cause of the evolution of oxygen when oxidisable gases are absorbed by permanganic acid." The results were such that it became desirable to obtain an aqueous solution of pure permanganic acid. It was decided to prepare this by dissolving the heptoxide in water. In an article by Morse and J. C. Olsen that appeared in 1900 occurs the following passage:

> (We) accordingly prepared a quantity of the anhydride by mixing potassium permanganate and concentrated sulphuric acid in vessels cooled by ice and salt. We soon learned, however, that something more than a low temperature is essential to safety in handling the product; for a minute quantity of the anhydride—certainly less than half a drop—which had been separated from the sulphuric acid, exploded with great violence and with disastrous results to one of us.[1] Some idea of the force of the explosion may be gained from the fact that one of the flying fragments of glass passed entirely through a burette which was mounted in the vicinity, leaving holes over half the diameter of the burette, edges of which were entirely free from cracks. After this experience, we decided to abandon the anhydride as a source of the acid, and to work out, if practicable, an electrolytic method of separating it from its salts.

The electrolytic method worked very satisfactorily, and led to the further use of this method in the preparation of osmotic membranes. The first results of this investigation are given in an article by Morse and D. W. Horn that appeared in 1901. They say:

> In this connection, it occurred to the authors that if a solution of a copper salt and one of potassium ferrocyanide are separated by a porous wall which is filled with water, and a current is passed from an electrode in the former to another in the latter solution the copper and the ferrocyanogen ions must meet in the interior of the wall and separate as copper ferrocyanide at all points of meeting, so that in the end there should be built up a continuous membrane well supported on either side by the material of the wall. The results of our experiments in this direction appear to have justified the expectation and to be worthy of a brief preliminary notice.

This marks the real beginning of the work on osmotic pressure with which the name of Morse will always be associated. But before the cells were available and therefore before any reliable measurements could be made, years of patient, skilfull work were still necessary. Difficulties that seemed insurmountable frequently arose and necessitated new efforts. It must be said that some of us in the laboratory, including myself, at times

[1] To make this story complete it should be added that Morse was the "one of us" here referred to. A piece of glass passed through the tissues of his neck in close proximity to the jugular vein. His escape from death was almost miraculous.

lost faith in the ultimate success of the work and were perhaps inclined to advise the use of cells that were not perfect. But Morse went steadily on. He had in mind a practically perfect cell that could be used for high pressures as well as low. He tried all sorts and conditions of clay and after many, many discouragements he succeeded in finding one and in making a satisfactory glaze quite different from any available, and he achieved success.

In 1902 he and J. C. W. Frazer described "The preparation of cells for the measurement of high osmotic pressures." A careful reading of this article will give some idea of the tremendous difficulties that were met and overcome. The closing paragraph may be advantageously quoted in this connection:

> The difficulties of construction are by no means completely overcome, and we have in view a number of changes which we hope will prove of advantage. That these difficulties are of great magnitude will be realized if one considers that in our last experiment the pressure which was measured and which was still below what we were called upon to control would suffice to raise a column of water at 20° to a point 15 meters higher than the top of the Eiffel tower, or which would raise from its base a marble shaft whose height is 120 meters. These comparisons will perhaps make it clear that the most painstaking attention to every detail of construction is absolutely essential to success when an apparatus like ours is to be made up of several parts, consisting of different materials, and which must be united without the usual mechanical means means of securing strong joints.

Soon after this the Carnegie Institution of Washington lent its powerful aid to the large investigation thus begun. In 1914 the institution published a memoir entitled "The Osmotic Pressure of Aqueous Solutions: Report on Investigations made in the Chemical Laboratory of the Johns Hopkins University during the years 1899–1913. By H. N. Morse." In it is given a detailed account of this remarkable piece of experimental work. Any one who reads it understandingly will recognize that no one but a master of experiment could have done this. The work required the highest degree of resourcefullness and skill, of patience and persistence. Any one of ordinary caliber would have stopped short of the accomplishment. Morse was not satisfied with anything but perfection as nearly as this could be reached, and as it never can be reached he worried about the residual no matter how small it might be. In the concluding chapter of the Carnegie Memoir occur these words:

> The work reported upon in the preceding chapters is only a fraction of the task which the author hopes to accomplish, or to see accomplished by others. The investigation—already fifteen years old—was undertaken, in the first instance, with a view to developing a practicable and fairly precise method for the direct measurement of the osmotic pressure of aqueous solutions. The need of such a method for the investigation of solutions seemed to the author very great and very urgent.

Honors came to him rather late but they came, the chief among these was the award of the Avogadro Medal of the Turin Academy of Sciences, in 1916.

> In 1911 an international congress of scientists assembled at Turin, Italy, to celebrate the centennial of the announcement of the hypothesis of Avogadro. Those in attendance decided to award a medal to be known as the Avogadro Medal. This medal was to be awarded to the investigator who should, in the judgment of the awarding committee, make the most valuable contribution to the subject of molecular physics during the years 1912, 1913 and 1914.

A few words in regard to Morse, the man. He was quiet and uneffusive. He did not care for the ordinary intercourse with his fellowmen. He lived, when not in the laboratory, for his family and a few kindred spirits. He married twice and had four children—a daughter and three sons. His second wife, who was Miss Elizabeth Dennis Clark, of Portland, Maine, his daughter and two sons survive him. In his later years his wife was of great assistance to him in preparing his articles for publication and was a true helpmate in every way.

For many years he spent his summers at Chebeague in the beautiful Casco Bay. Here

he had a simple comfortable cottage and a garden. He delighted to work, both in and out of the house, and this gave him his exercise. He was rather stout and he knew that he needed exercise to keep his weight down. He therefore indulged in walking, bicycling and finally in motoring, and he managed to keep fairly well. But, after his retirement in 1916, his health failed. His strength gave out and his courage also. He did not dare to take his car out of the garage, and his walks were very short. I saw him in May, just before he went to Maine, and thought he seemed more like his old self. He even talked of taking up his work again. It was not to be. I heard nothing from him after that. And then came the despatch announcing his rather sudden and entirely unexpected death. He was buried at Amherst, a place that meant so much to him—where he had spent his college years and for some time had had a summer home.

IRA REMSEN

WILHELM WUNDT, 1832–1920

THE death of Wundt removes the foremost figure of our academic world: a great man of science, a philosopher of repute, a prolific writer, a personality of extraordinary influence. Psychology, the science with which his name is permanently connected, was fortunate both in the date of his birth and in the length of his life. He came into the world a full decade later than Helmholtz and Virchow and Du Bois and Leuckart, Huxley and Tyndall and Spencer, the standard-bearers of science in the middle of the nineteenth century; so that, while his work and theirs overlapped, he still reaped the benefit of their pioneer labors. His length of days and the maintenance of his intellectual vigor not only enabled him to round off his manifold tasks—we all rejoice that the "Völkerpsychologie" is done, as we all rejoiced when Spencer published the final part of his "Synthetic Philosophy"—but also gave a much-needed stability to the young science of experimental psychology, whose name he coined and whose interest lay always nearest to his heart.

Wundt's outward life was uneventful. After a half-dozen years of study, principally in medicine, at the universities of Tübingen, Heidelberg and Berlin, he settled down as docent (1857) and assistant professor (1864) of physiology at Heidelberg, where Helmholtz held the chair of physiology from 1858 to 1871. In 1874 he was called as professor of philosophy to Zurich, and in 1875 was chosen in preference to Horwicz (who nowadays reads the once famous *Analysen?*) as professor of philosophy at Leipzig. Here he remained till the end of his life, gathering in his harvest of academic honors: the rectorship of his university, the honorary citizenship of the town, the order *pour la mérite,* the title of *wirklicher Geheimrat* of the kingdom of Saxony. He lived the simple family life of the older German tradition, and his days passed with the regularity of clockwork: the morning he spent on his current book or paper; then came the *Sprechstunde;* then, after the midday meal, his solitary constitutional in the park; then the formal visit to the laboratory; then the lecture; and then an informal gathering in the laboratory again. Wundt was an effective lecturer, and made no use of notes, though he always carried in his pocket a scrap of paper upon which notes had been made. He was devotedly cared for by his wife and, after her death, by his daughter, "meiner treuen Gefährtin im Urwald der Mythen und Märchen." His son turned some years since from philology to philosophy, and has written a valuable work upon Greek ethics.

Under these outward conditions, simple and sheltered, Wundt carried on his varied literary activities. If I were asked to pick out the most original and constructive items of his published work, I should name in the first place his "Beiträge zur Theorie der Sinneswahrnehmung" (1862), a rounded series of researches upon tactual and visual perception which contains in germ the doctrine of the later and better known *Physiologische Psychologie.* I should name, secondly, the *Untersuchungen zur Mechanik der Nerven und Nervencentren* (1871–1876), a solid bit of